On the Present State of the Philosophy of Quantum Mathematics

Howard Stein

The University of Chicago

It was with some trepidation that I agreed to speak today, because of a strong doubt that I could say anything substantial not already to be found in the literature of the subject. I cannot say that this trepidation has been subsequently relieved: all I can claim to offer in this paper is a review of certain basic characteristics or themes in the quantum-mechanical situation (which by now should, I think, be thoroughly understood by everyone engaged with the matter), supplemented by some rather general reflections on our philosophical predicament. In aid of these more general reflections, I shall indulge a proclivity for calling on historical matters--some fairly recent, some older, some ancient--which I hope may serve to place current issues in a useful perspective; and I ask your forgiveness for allowing myself to quote certain previous, but hitherto unpublished, remarks of my own.

In a talk delivered over a dozen years ago, in a symposium on theories of measurement, I offered an analogy--a little remote, but, I thought, instructive--between the status of that algebraic structure often called the "logic" of quantum mechanics (namely, at least in the simplest case, the set of all closed linear subspaces of a separable Hilbert space, with the operations of forming the orthogonal complement of an element and of forming the linear span of a set of mutually orthogonal elements), and the status in the later phase of classical physics of the "ether". The general point I had in mind was this: There is something in the theory that plays a quite central role in our formulation of it and our reasonings within it--a structure about which we can reasonably consider ourselves to <u>know</u> some important things, by this very fact of its role in a theory that forms a central part of our current knowledge. We also have, beyond this (if I may so call it) "positive knowledge" about the structure in question, a larger doctrine about the fundamental character or "nature" of this something: for the ether, it was the doctrine that the structure in question is that of a connected material system--according to the classical wave theory of light, some sort of elastic medium (although one which had to have

PSA 1982, Volume 2, pp. 563-581

quite unusual properties in order not to influence perceptibly the motions of planets and comets, and also to accommodate the details of the known laws of intensities and polarizations in reflection and refraction; in the latter part of the nineteenth century, with the advent and the eventual triumph of Maxwell's theory, the ether--now not merely "luminiferous", but "electromagnetic"--had to have a still more complicated constitution, and what that internal constitution was appeared to many of the great physicists of the time to be one of the truly fundamental problems of science. (Up to a point, as I shall maintain, they were right.) For the algebra of closed linear subspaces--or, equivalently, of self-adjoint projection operators--in quantum mechanics, the larger doctrine is that propounded first by von Neumann (1932, ch. iii, §5; 1955, pp. 247ff.) (or some modification thereof): that either all, or at least some, commuting sets of projections represent sets of possible "events", i.e., outcomes of possible experiments on a given physical system (more precisely: each single such set of commuting projections represents a set of possible outcomes of a single experiment).

So far, it will be observed, the indicated analogy is indeed remote, and is not conspicuously instructive. What makes it instructive in my opinion is that--again, in my opinion--there is good reason to believe our understanding of the physical significance of the algebra of projections to be appreciably less than our larger doctrine about it implies; just as, in the last century, physicists "knew" less about the ether than their larger doctrine implied. To press the point: I consider the attainment of a more coherent understanding of the physical meaning of this algebra to be one of the deep unsolved problems in physics today.

It happens that in the time since first expressing this view, I have published a piece of historical research on the theories of ether in the late nineteenth century (Stein 1981); and this study, combined with reflections on some of the more recent literature on philosophical problems of quantum mechanics, has reinforced my conviction that there is a significant analogy here. To explain this will be the main burden of the remarks that follow.

1) In 1966 there was published (some years late, through editorial inadvertence) a notable review article by John S. Bell, called "On the Problem of Hidden Variables in Quantum Mechanics"--by a striking coincidence, the same title, save for the initial preposition, as that of a famous (somewhat later) paper of Simon Kochen & Ernst Specker (1967), written quite independently and in a very different style, but whose contents are in all essential points already contained in the work of Bell. However, unlike Kochen and Specker, who thought their central result settled the problem of hidden variables, Bell pointed out that this is not at all the case: as Abner Shimony has elegantly put it (Shimony, forthcoming), "Bell, by a judo-like maneuver, cited Bohr in order to vindicate a family of hidden variables theories in which the values of observables depend not only upon the state of the system but also upon the context." This situation is, in general, now well known; the class of hidden-variable theories that escape the net of the Bell-

Kochen-Specker theorem has come to be called "contextual".

In my opinion, this term--reasonable enough in the circumstances--is in one way a little misleading. It is quite true that Bell (with full justification) invokes the view of Bohr to motivate his bringing in of a possible functional dependence of "values of observables" upon details of "experimental context." This very fact seems--altogether unjustifiably, in my view--to have led some who are hostile to the so-called "Copenhagen interpretation" of quantum mechanics to depreciate the contextual theories; or rather, I should say, to underestimate the force and significance of Bell's point, which cuts much deeper than merely to expose a flaw in the no-hidden-variables argument.

Moving back in history a little distance, to the heroic period of the creation of quantum mechanics in the middle 1920's, let me quote to you a brief extract from the admirable introduction to B. L. van der Waerden's admirable collection of fundamental papers, Sources of Quantum Mechanics (1967). First van der Waerden gives, in German, a passage from a letter of Heisenberg to Pauli (dated November 16, 1925--almost immediately following the completion of the decisive exposition of the theory by Born, Heisenberg, and Jordan); I translate:

> I have taken pains to make the work more physical than it was and am half satisfied with it. But I remain quite unhappy with the whole theory and was glad that you stand so completely on my side in your views about mathematics and physics. Here [in Göttingen] I am in an environment that thinks and feels in exact opposition, and I don't know whether I am just too stupid to understand mathematics. Göttingen falls into two camps: those who with Hilbert (or also Weyl in a letter to Jordan) speak of the great success that has been achieved through the introduction of the matrix calculus into physics; the others who, like Franck, say that one will never be able to understand the matrices. (p. 56).

And van der Waerden comments: "It appears from this letter that Heisenberg, although he himself had introduced matrices into physics and rediscovered the law of matrix multiplication, considered the application of matrix calculus to physics difficult to understand from the physical point of view." (p. 57).

The matrices of Heisenberg are, of course, to be identified with the self-adjoint operators of von Neumann's deeply illuminating papers of 1927, which offered an explanation of their physical meaning that has dominated most informed philosophic discussion of quantum mechanics to the present day: the operators represent "observable quantities", or "dynamical variables", in the sense that the so-called "spectrum" of an operator is the range of physically possible values of the associated quantity and the so-called "spectral projections" of the operator represent yes-or-no questions--or, equivalently, "propositions"--about the actual values; a "quantum state"--whether "pure" or "mixed"--consists in an assignment of probabilities to all these propositions (or, as I have elsewhere called them [Stein 1972], "eventualities"), to be considered as associated in the usual (however problematic) way with the frequencies of affirmative answers to these questions to be expected in experimental tests of the questions ("realizations", in my own version, of the eventualities) performed on large numbers of systems in the same given quantum state.

There seems to me very little doubt--although, committed to a modest, or Socratic, philosophic skepticism, I must say on general principles that there is some doubt--that this doctrine of von Neumann's possesses a very important kernel of truth (comparable, in the analogy I have posed, to the kernel of truth in the nineteenth-century doctrine of the ether). I shall return to this presently; but what I now wish to point out is that the "contextual" considerations adduced by Bell can be expressed quite independently of the Copenhagen interpretation, and quite independently of the issue of hidden variables: what Bell has pointed out in effect is that however suggestive, important, and deep von Neumann's analysis of quantum mechanics was, it has by no means been proved that it is really correct to regard the projections of von Neumann as literally representing "propositions about"--or possible "properties" of--a quantum-physical system: the "contextual hidden-variable theories" simply exemplify a way in which that interpretation of the projections could fail, while all the empirical statistical predictions of quantum mechanics could be maintained. In other words--in view of the fact that the interpretation of the matrices, or operators, in general, can be seen as resting squarely on that of the projections--one is not justified in regarding the early mystery about the physical significance of the matrices, illustrated by the perplexity expressed by Heisenberg, as having been entirely dispelled by the analysis of von Neumann (any more, I should say, reverting to the analogy, than it would have been correct to say in the period from 1822 or so to 1865, when Maxwell's theory opened vast new prospects, that Fresnel's work--however deeply clarifying of the physical nature of light--had entirely elucidated the character of the postulated luminiferous ether).

2) The possibility exemplified by the contextual theories is not the only reason for reserve about the literal acceptance of von Neumann's interpretation of projections as representing testable attributes. Let us not now concern ourselves with the details of von Neumann's account of the process of "testing"--or "measurement"--itself: this has been much discussed, and it may be taken as agreed that what von Neumann demands of a process that is to qualify as a "measurement" is unrealistic. But consider what is almost the weakest possible requirement compatible with von Neumann's basic interpretation of projections on the one hand, and states on the other. This is the requirement that--for a given, "testable" eventuality *e*--there be some specifiable experimental arrangement, whose outcome can be described as "yes" or "no", such that quantum mechanics predicts, for any quantum state *s* of the system being tested, a probability for the "yes" outcome equal to the probability assigned by the state *s* to the eventuality *e*. Now, I have remarked, in a paper published a decade ago

(Stein 1972, pp. 411-412), that there are reasons of principle to deny that even this condition can literally be met for any non-trivial eventuality e (that is, for any but the extreme cases of the logically determinate: the projection 0, to which corresponds the eventuality for which the answer is "No, no matter what", and the projection 1, to which corresponds the contrary case: "Yes, no matter what"). In brief, the point is, first, that "all possible states s" include all possible locations of the system; but one has to know something about where the system is in order to perform a physical test upon it. This first remark suggests a further weakening: to allow the specification of experimental procedure to depend upon the state s, as well as upon the eventuality e. But again, this obviously will not suffice, since there are states s--and, indeed, these are the typical quantum states--that place no strict bounds upon the possible location of the system. Finally, even if one goes further and imposes the requirement only upon the eventuality e and certain special states s, the attempt fails when one notes that physical tests always depend for their feasibility not only upon some knowledge of where the system is, but also upon some restriction on its state of motion; whereas quantum mechanics does not admit any physical state that entails simultaneous strict bounds--however wide--upon the position of a system and its momentum.

3) These considerations can lead to a kind of vertigo. How is it, in view of what has been said, that (a) quantum mechanics is able to make any empirical predictions, and (b) more specifically, something like the von Neumann interpretation of the theory can guide those predictions? In two earlier papers--the one just alluded to, and a much shorter one (Stein 1970) (written later but published two years before)--I have discussed this point and have emphasized that the actual application of the theory is not really problematic. The reason (here, by the way, I unashamedly distinguish between theory and observation) is that in the first place what one observes is always some attribute of a macroscopic system. For such a system--a piece of laboratory apparatus, say--although in principle the previous remarks apply, there do exist states--and now these are the "typical" ones--in which position and motion, while not simultaneously strictly limited, are nonetheless limited with such high probability that one is fully justified in ignoring, in practice, what I may call (with intentionally double meaning) the "outside chances". In the second place, as to the system with which the apparatus interacts, in favorable cases--namely, the ones that do allow experiments--a similar situation will obtain: the quantum state s will, in the probabilistic sense, be sufficiently well restricted in the relevant respects to allow one to proceed in practice.

4) This situation has induced in many--one's impression is, in the vast majority of physicists--an attitude of complacency about quantum mechanics: an attitude that is epitomized by the crude slogan, "A theory is just a device for predicting the results of experiment", supplemented by the gloss that quantum mechanics is a device that works very well. The last part of this remark is certainly true, and is quite sufficient to justify confidence in the theory when it is so

used. But I should like briefly to compare this dismissive response to problems that may be deemed "metaphysical", or "mere philosophers' problems", with a pair of analogous responses from the late nineteenth century. The first case is very well known, even notorious; but its moral seems often forgotten: namely, the repudiation by such empiricists as Mach of atoms as anything more than a "device", and associated with that view of atoms the depreciation of attempts to determine their "real properties" (e.g., mass or internal structure) and "real effects" of their supposed existence as the pursuit of a will-o'-the-wisp (cf. Mach 1883, ch. iv. §4, para. 9; 1960, pp. 532-3). The second case is, I think, less well known and more interesting: it concerns the ether, and I have touched upon it toward the end of the paper on that subject mentioned previously (Stein 1981, pp. 332-337), and in more detail in talks which have not been published. Here the central figure (for my purposes) is Henri Poincaré, who adopted a very reserved position on the question of atoms, and a perhaps more emphatically negative view concerning the ether. Now, this negative view is rather in accord with current prejudices: we "know" that the ether has been proved to be a mere fiction, and that the great labors of all the nineteenth-century physicists who strove to construct a viable "model" of the ether have proved sterile. However, this "knowledge" of ours is only a half-truth, as I shall try briefly to explain.

Poincaré himself considered the ether to be a fiction; but he did _not_ attempt to _remove_ that fiction from the formulation of electrodynamic theory: on the contrary, he regarded it as indispensable, since it was the only means by which Maxwell's theory of the electromagnetic field could be represented as a Newtonian dynamical theory (a mode of representation that Poincaré deemed a part of the definitive, although--or rather _because_--"conventional" constitutional framework of physics). Since he nonetheless did consider it a fiction, he thought it superfluous to try to construct a detailed picture of the ether as such a Newtonian medium (i.e., a system of interacting bodies), and proposed instead that the conventional or "constitutional" requirements be seen to be fully satisfied by an _existence theorem_: the theory of Maxwell, Poincaré tells us, has been shown to allow a dynamical formulation of Lagrangian type; and Poincaré offers a theorem stating that any such theory admits (not merely one, but) infinitely many strictly Newtonian "models" or substructures. With this result, he says, all legitimate demands are met; we should not be deluded into searching for the "real" substructure (of such questions, he suggests that it is best to "leave them to the metaphysicians", and immediately afterwards implies that they can never be answered--thus giving the back of his hand simultaneously to the questions _and_ to the "metaphysicians" [Poincaré 1890, pp. ix-xv; cf. also 1902, pp. 217-223]).

I cannot resist a side-remark here, although it is of no importance for our present concerns: Poincaré's theorem is true, but his proof is fallacious; indeed, the proof is essentially trivial, but the theorem--from a purely mathematical point of view--is a deep one. It is entirely equivalent to the theorem of John Nash (1956) that every Riemannian manifold admits a smooth isometric embedding into some Euclidean space;

the triviality of Poincaré's argument comes from the fact that he considers only the problem of *local* embedding. What is more to the point, the *deep* theorem is quite irrelevant to the physical situation as we now see it, since although the classical electromagnetic field is still seen to be a dynamical system in the sense of Lagrange (or in a more generalized sense--adapted to systems of infinitely many degrees of freedom, and to relativistic space-time)--and although this is a circumstance of considerable importance for the quantum-field-theoretic transformation of the classical theory--any question of a strictly Newtonian substructure has long since proved entirely nugatory.

So far, to our representative physicist of the so-called "instrumentalist" persuasion, this may seem mere ideological history, with no instructive bearings: Poincaré was right to consider the ether a fiction, but wrong to place such importance on the possibility of Newtonian underpinnings. I shall return to Poincaré in a moment; but I have promised at least one ancient reference, and I should like to make good on that now: We have learned, from a series of remarkable historical investigations of the past hundred years (cf. Neugebauer 1957, ch. v, and 1975, Book II), that the Babylonian astronomers of the Seleucid era possessed a highly refined set of algorithmic procedures for predicting astronomical phenomena. These procedures were entirely independent of any assumptions about the nature of what *we* call the "celestial *bodies*"--they can be taken as dealing exclusively with the phenomena to be seen in the sky, and with the regularities of recurrence of these phenomena. Now, there are two points of view from which one might consider this Babylonian theory as having an important limitation. The first is what might be called the point of view of "simple realism": we should like to know more than just when to expect the moon to be eclipsed--we should like to know what the moon itself *is*, how it "really" moves, what "causes" an eclipse, and "why" eclipses exhibit the pattern they do. One might imagine a Babylonian instrumentalist rejecting these as mere philosophers' questions, pointing out that the moon is beyond reach and that hypotheses about what it "really is" ("shining with borrowed light", according to Parmenides; "a stone", or "of earth", according to Anaxagoras [cf., Burnet 1930, pp. 177, 271]) or how it "really moves" are at best convenient fictions. But of course the history of science is, among other things, a history of expanding "reach": we can now *travel* to the moon, and the attendant *phenomena* form a part of what our present theoretical armamentarium, conceived just as an "instrument", is concerned with. It is salutary to remind ourselves how recent this particular extension of our reach is--and how deeply involved a role the development of *theory*, or "speculation" (*theōría* and *speculatio* are strictly equivalent words), has played in making such an achievement possible. And thus we come to the second point of view from which the Babylonian perspective can be seen as too confining: it is the point of view of a sophisticated "instrumentalist", who does not dismiss as otiose questions that arise within, or about, a theory, that may lie beyond our present "instrumental" control of phenomena, precisely because he wishes to expand that control, and because the exploration of--to put it colloquially--the "far out" implications of theories has proved by far our most powerful resource in achieving such expansion. (I have elsewhere argued--in, I fear, yet another unpublished talk--that in this sense the legitimate, as opposed to merely ideological, claims of "realism" and those of a "sophisticated instrumentalism" converge, and consist effectively in the demand that theories and the questions they give rise to be taken seriously on their own terms, and not dismissed as something "mere".)

To come back, then, to Poincaré: his dismissal of the ether as a "fiction" led him to discount the notion that the electromagnetic field might possess what he thought of as genuinely dynamical attributes--momentum, for example--as itself a harmless fiction, but without real signficance for physical understanding.[1] I need hardly point out that this was a fundamental error: one of several that occur, in absolutely fascinating association with deep remarks that contributed significantly to the advancement of the subject, in Poincaré's work on the electrodynamics of moving bodies (see Poincaré 1895 and 1900). The brief moral I preach from this is that dismissive instrumentalism--as opposed to what I have called "sophisticated instrumentalism"--can be an impediment to the advancement of knowledge.

5) Having, in the preceding section, criticized simple-minded instrumentalism, it is only fair to do the same for simple-minded realism. The hard-nosed realists of the late 19th century were the strict mechanists, of whom Kelvin may be taken as the leading example. Kelvin's attempts to construct a satisfactory mechanical model of the ether (cf. Stein 1981) began as early as 1847, when he was plain Mr. Thomson (and nearly two decades before Maxwell's theory); continued through his distinguished career as Sir William Thomson (1866-1892), and his last fifteen years (1892-1907) as Baron Kelvin: his final paper on the subject was published in the year of his death. The series of speculations became, in some respects, increasingly bizarre; for instance, Kelvin was led to abandon the principle of the impenetrability of matter (which is, we may recall, what Newton regarded as its essential attribute). It is not, however, either the occurrence of such a long series of attempts (which has parallels, e.g., in Mendeleëv's work on the periodic table, or in the long speculative development of the atomic and kinetic-molecular theory), or the "strange" character of the hypotheses (for the "positive" conclusions we have come to about the ether--or electromagnetic field--are, from the older point of view, even more strange) that give to this side of Kelvin's work the earmarks of what Imre Lakatos (cf. Lakatos 1970) called a "degenerating" program. It is rather that, although Kelvin made many important contributions to our understanding of natural phenomena, his speculations on the ether were uniformly devoid of any significant connection with problematic phenomena of optics or electromagnetism. This is true, of course, of other model-builders as well as Kelvin; but by no means of all physicists who took the conception of ether seriously in the way Poincaré did not. In the honor roll of theorists whose ethereal speculations did lead to confrontation of phenomena, and did serve to advance understanding of phenomena, there may be mentioned as conspicuous instances: Young and Fresnel, Maxwell, Helmholtz, G. F. Fitzgerald, H. A. Lorentz, and

Heinrich Hertz. I should like to cite, as an epitome of the philosophical spirit that I see as presiding over this constructive line of ether theory (just the spirit I earlier characterized as "modest skepticism"), a succinct question raised by Maxwell in a two-page note in 1876 (long after his formulation of the electromagnetic theory of light): he says (I paraphrase slightly) that there is a great need to find a way to express "exactly what is known" about the wave nature of light. No one familiar with Maxwell's work can be in any doubt that the importance of such a formulation, for him, would lie in its making more precise our knowledge of what we do _not_ yet know--i.e., what we need to try to find out.

6) Back, at last, to quantum mechanics: I have said that it is almost beyond doubt that an important kernel of truth is contained in von Neumann's doctrine about the algebra of projections (as, in the "mechanical" theory of the ether, there was embedded the truth that the electromagnetic field can and should be regarded as a dynamical system, possessing such attributes as momentum and angular momentum, and--what is more--even exercising gravitational influence). Part of the reason for this conviction is just the role von Neumann's analysis, despite its weak points, has played in guiding the actual procedure of "applying" the theory. But, as I have discussed at length in one of my published papers on the subject (Stein 1972), there is another--theoretically, or at least mathematically, deeper--reason for taking very seriously the doctrine of "eventualities" and their relation to "states".

It is well known that the theorem of Bell, Kochen, and Specker, already mentioned, is a corollary of a theorem proved in 1957 by A. M. Gleason. (One sometimes hears it said that the existence of a _finite_ counter-example, established by Bell and by Kochen-Specker, is more than Gleason's theorem implies. That is not quite true: a standard "compactness" argument starting from Gleason leads to the existence of such a finite example, although not to the exhibition of a _specific_ one. But this merely by-the-bye.) Now, Gleason's theorem is mathematically quite deep--its proof notoriously intricate--and what it shows is that, given von Neumann's basic postulate that a quantum-statistical state is a probability distribution (or "measure") on the algebra of projection operators, all possible such states are precisely exhausted by the set of those that von Neumann himself considered: those defined by the so-called "von Neumann density operators", among which there occur as the distinguished cases of maximal specificity the familiar "pure states" represented by "rays" in the Hilbert space (or, slightly more loosely, under suitable conventions and with a small degree of arbitrariness, by the "wave-functions" of Schrödinger).

Next, with this much established, a series of considerations of group-theoretic character, bearing partly upon the general form of the _dynamics_ to be expected for such a system (in which the crucial thing is what is called a "representation", acting upon the space of states, of the additive group of the real numbers--construed as "time-intervals"), partly upon purely geometric "symmetries" (in which the corresponding "representations" are those of the group of symmetries, i.e., the congruence-group, of Euclidean space), and at the most involved and rewarding level bearing upon both of these together ("invariance of dynamical laws under the symmetries of space-time") leads to very far-reaching physical conclusions: for instance, to the classification of particles by their "intrinsic angular momentum" or "spin"--a notion entirely foreign to classical physics; to the Heisenberg commutation relations; and (as the work of George Mackey [see, especially, Mackey 1978, pp. 159-234] has shown), when the requirement of space-time invariance is fully exploited for a finite system of particles of given spins and masses, to the detailed form of the law of dynamical evolution (the "time-dependent Schrödinger equation"), in which only the choice of the potential-energy operator remains undetermined. These results, although still at a high level of generality, are very powerful; for with one amendment--the symmetry rules for "bosons" and "fermions" (i.e., particles of integral and half-odd-integral spin, respectively), and one approximative specification--the choice of a potential energy expressing just electrostatic force--one already gets an approximate, qualitatively correct account of the existence and properties of such stable physical structures as atoms, molecules, and crystals (hence: solid bodies): an achievement that had long seemed just within the grasp of classical physics, and yet always eluded it.

It is possible that this intimate connection--mediated by considerations of symmetry or dynamical invariance--of von Neumann's doctrine with the physically important consequences of quantum mechanics is in some sense an accident. Similarly, the opponents of the wave-theory of light up to the 1820's surely believed that the successful prediction of the law of double refraction for uniaxial crystals by Huygens on the basis of his tentative and very unsatisfactory theory of the process in the ether constituting light was a lucky accident. On this possibility I have two comments. The first is objective, but very general: as the case of Huygens's long ignored theory illustrates, it is better for the progress of science that an interesting connection--even though it may be accidental--not be overlooked. The second is more personal and subjective: committed to a modest skepticism, I am prepared to be surprised; and the development of science certainly encourages one to expect surprises--to suspect that "there are more things in heaven and earth than"--at any given stage--"are dreamt of in your philosophy"; but I should be profoundly surprised--I might almost say, shocked at nature's irresponsibility--if so striking a system of connections as the one I have just sketched turned out to be merely fortuitous. I therefore hold to the hypothesis that these connections are a very important clue to further understanding of the nature of the world; and since symmetries of space-time and the concept of a dynamical law play a central role in the connections, I am tempted to expect that we may yet have surprising things to learn about the concept of space-time, and about the character of what we are accustomed to call "causation".

7) Speaking in this same vein of subjective conviction, I have to say

that a considerable part of the technical work that has been devoted to the philosophical problems of quantum mechanics seems to me to show the same symptoms of aridity as the work of Kelvin on the ether, or as Poincaré's appeal to the existence theorem of Nash as solving the philosophical problems of Maxwell's electrodynamics. The problem of the reduction of the wave packet--I take it that what this is is too well known to require any elucidation here--remains what it always has been: baffling.[2] As I have commented before in print, the "Copenhagen" view about quantum mechanics does not succeeed in either eliminating or solving this problem (Stein 1970, p. 102; cf. also 1972, p. 436, n. 40). Again, as I have also remarked before (Stein 1972, pp. 419-420), the view advanced by H. W. Everett (1957)--that there simply is no reduction--quite fails to account for the actual phenomena of physics, which we may summarize as the appearance of reduction: namely, our experience of definite observations. To amplify slightly: Everett supposes that the evolution of the world "branches" in such a way that all quantum-mechanically possible outcomes of experiments actually occur, but that any particular subjective experience--e.g., mine right now--is confined to one branch. Setting aside other possible objections, the crucial difficulty is that neither quantum mechanics, nor what Everett has to say about it, gives us any criterion for the circumstances in which such "branching" occurs; and this itself is tantamount to a mere repetition of the problem of reduction of the wave-packet (that is, "branching" is substituted for "reduction"; but no clarification is achieved). A view suggested some years ago by Simon Kochen (1979)--that the effect of interaction of a pair of systems is to single out a distinguished Boolean subalgebra of the algebra of all eventualities, and that it is the members of this subalgebra that "bear truth-values"--i.e., represent the bona fide "definite attributes" in such a coupled system--could be viewed as providing Everett's interpretation with the missing criterion. Unfortunately Kochen's suggestion seems to me to founder upon considering that the world to which we want to (and do) apply physics does not come divided up into well-defined and separated pairs of interacting systems (and the decomposition theorem for vectors in tensor products upon which Kochen's analysis depends is restricted to tensor products of pairs of spaces). In fact, the natural way to regard Kochen's "pair of systems" is as "test system" and "apparatus"--which would seem to take us back to Copenhagen. Finally (in respect of this issue of wave-packet-reduction), the arguments that have been made--e.g., on thermodynamic grounds--for "in principle undetectability" of what might be called "unorthodox" eventualities as a basis for explaining a "practical reduction" not only fail to deal with the issue in principle but have an uncomfortable vagueness--and are reminiscent, to me, of Huygens's argument that there would be no "detectable" spreading of light waves into the geometric shadow (or "not enough motion there to constitute light"): it was in fact only when detectable effects--phenomena--of the kind Huygens tried to argue away were seriously studied, that a deeper understanding of the wave nature of light was achieved.

8) One most notable case of work on the problems of quantum mechanics that has not been devoid of contact with phenomena is the famous result

of Bell (1964) concerning the Einstein-Podolsky-Rosen paradox, and, of course, the experiments stimulated by that result--in the first instance, those of Clauser, Horne, Shimony, and Holt (1969). A striking feature of this work is that the theorem of Bell in question here--in contrast with that of Gleason, or even of Bell-Kochen-Specker--is not deep at all, but (mathematically) quite superficial; yet it remained undiscovered through nearly four decades of consideration of the problems of quantum mechanics--until Bell, with his characteristic penetration and lucidity, asked the right question about the physics, and found a physically significant answer.

Bell's result has now been before us for some eighteen years, and much technical discussion has grown up around it. I shall not attempt to add a word to that technical discussion, but shall confine myself again to more general comment. (More concentrated analysis is of course promised by the symposium to follow shortly.) First, it should be obvious to anyone that no finite collection of observational data can rigorously exclude the possibility of underlying determinism. Long before Duhem, it was clear to Newton that empirical arguments--what he called "propositions inferred by general induction from phenomena"--could always be "evaded by hypotheses"; and clear to him, also, that such evasion is destructive of the enterprise of science. We unfortunately still do not possess a precise general criterion of sound inductive argument, or (on the other hand) of the "evasiveness" of hypotheses; but we _do_ possess a rather good _working_ criterion: in physics, we have no serious difficulty in recognizing when we have an honest-to-God theory with honest-to-God consequences--or even, as in Einstein's quantum paper of 1905, or in the time of the old quantum theory, characterized (in a phrase of van der Waerden's (1967, p. 8)) by "systematic guessing", when we have a heuristic principle that can lead to honest results. To give the cheapest, crudest example of the opposite--the evasive hypothesis--one could always suppose the phenomena to have been rigged by a sort of Cartesian demon: just as a determinate computer program can _simulate_ a finite "random" process, so the world could be "determined" by a mechanism behind the scenes to produce just the phenomena we have observed. Although no one has actually proposed a hypothesis of quite this degree of crudeness, some attempts to argue for the possibility of assigning definite truth-values to all von Neumann "propositions" at all times have seemed to me not very far from that. A possibility of greater subtlety has been pretty well aired in connection with the Bell results: that of prearrangement, as it were, not "behind the scenes", but _within_ the world as we conceive it in space-time, by previous interaction of the spatially separated pieces of apparatus in the situation of Einstein-Podolsky-Rosen. It should be obvious that this possibility does indeed _always_ exist: it _cannot_ be excluded, for instance, by arranging an experiment in such a way that crucial events affecting the pieces of apparatus at the times of measurement have space-like separation, because the past light-cones of _any_ two events overlap. But although this is a subtler point than the possibility of programming "behind the scenes", it is no less evasive: for it is a bare, naked possibility, devoid of the slightest suggestion of how a _theory_ of such interactions accounting for the actual phenomena could be found. Some words of Bell are worth quoting here (they are taken from a paper defending a special point that has proved not quite correct, but that is irrelevant to their aptness); speaking of the possibility of "conspiracy within the overlap of the light-cones," Bell remarked (1977, p. 83): "A theory may appear in which such conspiracies inevitably occur.... When that theory is announced I will not refuse to listen, either on methodological or other grounds. But I will not myself try to make such a theory." I think it fair to conclude, at this point, that Bell's theorem together with available experimental results--and with the background of our well-established conception of the local Einstein-Minkowski structure of space-time--provides as convincing a refutation of the possibility of underlying states with definite properties evolving in deterministic fashion, as Hertz's experiments provided a convincing support for the electromagnetic field theory of Maxwell (and concomitant refutation of the possibility of a tenable theory of electromagnetic action at a distance).

There remains one possibility of more serious interest: namely, that of a theory that postulates underlying states with definite properties at all times, but evolving _stochastically_ rather than deterministically. Recent work (Hellman 1982; Jarrett 1983) has seemed to suggest that although this possibility remains open, arguments related to Bell's impose constraints upon the laws of stochastic evolution of such a kind that it is hard to see how to formulate laws satisfying these constraints within the local Einstein-Minkowski structure. At this stage, then, the indicated possibility is almost as "naked" a one as that of "conspiracy within the light-cones". It is perhaps not _quite_ as "naked", simply because there has been less exploration--indeed, so far as I am aware, _no_ serious exploration--of what an honest-to-God straightforward stochastic theory of fundamental dynamical evolution of physical systems would look like. But this is hardly very encouraging: it means that, up to now, no one has had any idea how to build such a theory.

9) Perhaps I should stop with that: having, I fear, said little, I fear I have little to add. Still, there are a few points I should like to make about the present state of our knowledge as I see it, and I shall trespass a little more on your patience in order to make them.

First, I think that we--perhaps physicists, and certainly philosophers--too often fall into the trap of presuming that we have an adequately clear concept of what is called "causation". I have never considered Hume's analysis of the notion of "cause" a satisfactory one; but I think that Hume was quite right in maintaining that whatever we know of causes--or, as I should prefer to say, of causal nexus, or interconnection, or interaction--we know not _a priori_ but only on the basis of experience; and I should add that our best such knowledge is contained precisely in our best scientific theories. Thus Newton's theory of gravitation taught us something quite new about causal interconnection: that it need not be mediated by bodies in direct contact. And special relativity taught us something further: that the form of a law of fundamental interaction in our world cannot be that of the

Newtonian paradigm, because the structure of space-time does not allow us to formulate a law in the way Newton did. Of course, the confrontation of these two results--Newtonian gravitation and Einstein-Minkowski space-time--posed a problem, resolved by the general theory of relativity. Now, there is no room for doubt that our best present physical theory is the quantum theory. One way to express the significance of Bell's results is to say that they illuminate an aspect of what is radically new in what the quantum theory teaches us about causal interaction, by demonstrating the incompatibility of confirmable statistical predictions of quantum mechanics with the kind of causal interaction envisaged by such a classicist as Einstein.

Second, because I consider the issue of wave-packet reduction to be both serious and entirely unresolved--in particular, because I think we have at present no serious evidence one way or the other on the question whether there is truly in nature any such process as "reduction"--I believe that speculation of a general kind on the philosophical implications of reduction should indeed be admitted into consideration, but on the other hand should be considered only with great caution and reserve. Thus, the view that reduction may in some way be occasioned by "mind" or "sentience" ought not, as I see it, be treated with contempt (although issues of locality seem to make such a hypothesis harder even to formulate coherently than it formerly appeared to be); but it should be recognized (setting aside the parenthetical remark just preceding) that this view is doubly speculative, in that we do not know that reduction "occurs" at all, hence we know still less that its putative occurrence is occasioned by anything to do with consciousness. And in a similar way the notion (attractive in itself) recently discussed by Shimony (1978, 1980; cf. also Heisenberg 1958, p. 53) that the radically new idea introduced into physics by quantum mechanics is that of "the passage from potentiality to actuality" ought to be entertained only with the reservation that we do not yet know of any clear case that can be characterized as such a passage.

Third, the fact the the problem of the reduction of the wave-packet has remained baffling for so long, and that we lack any clue at present to its solution, seems to me no reason at all either for despair, or for embracing what (in my view, as I have tried to explain) are glib and inadequate pseudo-solutions. Real problems are not always ripe for solution. There has repeatedly occurred a conviction that "we"--whether Aristotle and his followers, Descartes and his followers, Kelvin and company, or whoever--have either just achieved, or stand on the brink of, the final essential enlightenment. Such convictions have always been delusive. They may one day be true: consistent--or "extreme"--modest skepticism forbids me to deny the possibility. But there is little reason to suppose that conviction (which some apparently do possess) to be true today. There are many aspects of the world that we by no means understand. Quantum mechanics has contributed greatly--through molecular genetics, as Schrödinger as early as 1944 suggested it might--to our understanding of biological process; yet it would perhaps be excessive to claim that we now fully understand the difference between what we call "living" and "non-living"; and we certainly do not understand (what Schrödinger [1944, p. 7] believed we never could) the physical bearings of conscious processes--an issue that I should certainly not want to see dismissed as a pseudo-problem. What surprises future developments in these domains may bring is of course unforeseeable--by the definition of surprises. But it can certainly not be excluded that such developments may involve alterations in our fundamental physical conceptions comparably radical with those that led to our present highly developed understanding of atomic structure and chemical processes.

Fourth, I have conspicuously failed so far to mention the case in which our presently formulated physical theories are in most obvious and exigent difficulty--the global case, that of the theory of the whole cosmos. The problem has several aspects. The Copenhagen interpretation--or the standard, practical, working view--of quantum mechanics seems quite helpless in this realm. Indeed, to circumvent this problem was a principal motivation for the interpretation suggested by Everett; but attempts to exploit Everett's view to develop a quantum cosmology have not succeeded. In any case--problems of interpretation aside--we do not possess a satisfactory general quantum theory of the interaction of greatest moment on the cosmic scale, namely gravitation. Again, the symmetries of space-time, which as I have said play so strong a role in the structure of quantum theory, are essentially local (or, more strictly, "infinitesimal") symmetries; it surely seems likely that their failure in a global sense must have deep implications for any extension of quantum mechanics to the cosmos--or for any revision of quantum mechanics that might make such an extension possible.

There is one reason, of the utmost simplicity, for hoping that a successful global field theory would have direct implications for the question of reduction of the wave-packet. In the usual schematization of the process of measurement, following von Neumann, we consider a pair of coupled systems--"test-system" and "apparatus" (cf. my earlier comments on a view of Kochen's). It is generally understood that when we look at the apparatus, we have to include ourselves in the quantum-mechanical coupling--i.e., in the tensor product constituting the state-space. But even if we do not look, the system and apparatus together are not an isolated coupled system. Indeed, looking is mediated by electromagnetic interaction (as, for that matter, is touching, or any other mode of sensing we possess); but whether we look or not, every system is always coupled to the electromagnetic field. Furthermore, the electromagnetic field constitutes by its own basic nature a single global system--and so does the gravitational field (or metric-field of space-time), to which also all physical systems are coupled. In this way, it seems to me, the issues that bear upon reduction of the wave-packet are substantially connected--not just by philosophical lucubration--with what I have referred to as the most obvious and exigent difficulty confronting physical theory. We have no guarantee that this difficulty will be solved--in our lifetime, or in that of the human race. But science has never had such a guarantee. One thing, at any rate, we do know--as well as the nature of things (including us) permits us to "know" such matters--thanks to the work of Bell: that if such a solution is found, it will not be of the kind hoped for and expected by Einstein.

Notes.

[1] I am indebted to Professor Fritz Rohrlich, of Syracuse University, for pointing out to me that in his famous paper of 1906 Poincaré does make use (1954, p. 524) of electromagnetic momentum; his treatment of it here contrasts with that in his earlier article (1900), where it is introduced as a pure fiction. Yet it ought to be noted that this great work of 1906 is itself introduced by remarks suggesting that Poincaré has deep reservations about its physical significance; and that in his review article of two years later, in discussing the "failure of the principle of reaction" in the "new mechanics" (1908a; in 1954, pp. 567-570; in 1908b, pp. 222-225), he fails to mention at all the concept of field momentum and the fact that when the latter is taken into account conservation of momentum remains valid.

[2] In the discussion following this talk, it was suggested that the application of quantum mechanics to statistical aggregates (or "ensembles") of systems can be made without any appeal to a "reduction of the wave-packet". With this statement I have no quarrel; but I have touched previously (1970, pp. 100 and 102; 1972, pp. 413-420) on the reasons why in spite of this I believe that a problem remains. The present note is not an appropriate place to discuss the matter in detail; but I should like to emphasize the following points: I do not take it as clear that von Neumann's distinction (1932, p. 351) between "process (1.)" ("reduction of the wave-packet") and "process (2.)" (evolution in accordance with the Schrödinger equation) corresponds to a real physical distinction: whether this is so I regard as part of the problem. (b) The view that the occurrence of probabilities in quantum mechanics renders the theory applicable only to "aggregates", not at all to single systems, seems to me both incoherent in formulation, and false to actual practice (e.g., we use the theory to predict the properties of individual chunks of matter). (c) Moreover, to assume that we cannot use quantum mechanics except where, in principle, many exemplars of a system are available, is to exclude in advance the possibility of a unification of quantum mechanics and general relativity; and as I remark in section 9 of the present paper, although we may never succeed in effecting such a unification, to adopt a counsel of despair in the matter appears counter to the interests of scientific understanding.

References

Bell, John S. (1964). "On the Einstein-Podolsky-Rosen Paradox." Physics 1: 195-200.

------------. (1966). "On the Problem of Hidden Variables in Quantum Mechanics." Reviews of Modern Physics 38: 447-452.

------------. (1977). "Free Variables and Local Causality." Epistemological Letters 15: 79-84.

Burnet, John. (1930). Early Greek Philosophy. 4th ed. London: Macmillan Company.

Clauser, John F.; Horne, Michael A.; Shimony, Abner; and Holt, Richard A. (1969). "Proposed Experiment to Test Local Hidden-Variable Theories." Physical Review Letters 23: 880-884.

Einstein, Albert. (1905). "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt." Annalen der Physik (series 4) 17: 132-148.

Everett, Hugh, III. (1957). "'Relative State' Formulation of Quantum Mechanics." Reviews of Modern Physics 29: 454-465.

Gleason, Andrew M. (1957). "Measures on the Closed Subspaces of a Hilbert Space." Journal of Mathematics and Mechanics 6: 855-893.

Heisenberg, Werner. (1958). Physics and Philosophy. New York: Harper & Brothers.

Hellman, Geoffrey. (1982). "Stochastic Einstein-locality and the Bell Theorems." Synthèse 53: 461-504.

Jarrett, Jon P. (1983). Bell's Theorem, Quantum Mechanics, and Local Realism. Unpublished Ph.D. Dissertation, University of Chicago.

Kochen, Simon. (1979). "The Interpretation of Quantum Mechanics." Unpublished typescript; forthcoming in Advances in Mathematics.

------------- and Specker, Ernst. (1967). "The Problem of Hidden Variables in Quantum Mechanics." Journal of Mathematics and Mechanics 17: 59-87.

Lakatos, Imre. (1970). "Falsification and the Methodology of Scientific Research Programs." In Criticism and the Growth of Knowledge. Edited by Imre Lakatos and Alan Musgrave. Cambridge: Cambridge University Press. Pages 91-196.

Mach, Ernst. (1883). Die Mechanik in ihrer Entwicklung historisch-kritisch dargestellt. Leipzig: F.A. Brockhaus. (Reprinted as The Science of Mechanics: A Critical and Historical Account of Its Development. (trans.) T.J. McCormack. La Salle, IL: Open Court, 1960.)

Mackey, George W. (1978). Unitary Group Representations in Physics, Probability, and Number Theory. Reading, Mass.: Benjamin/ Cummings Publishing Co.

Maxwell, James Clerk. (1876). "On the Proof of the Equations of Motion of a Connected System." Proceedings of the Cambridge Philosophical Society 2: 292-294. (As reprinted in Niven, W.D. (ed.). The Scientific Papers of James Clerk Maxwell, Volume II. Cambridge: Cambridge University Press, 1890. Pages 308-309.)

Nash, John. (1956). "The Imbedding Problem for Riemannian Manifolds." Annals of Mathematics 63: 20-63.

Neugebauer, Otto. (1957). The Exact Sciences in Antiquity. 2nd ed. Providence: Brown University Press. (As reprinted 1969. New York: Dover Publications.)

----------------. (1975). A History of Ancient Mathematical Astronomy. Part One. New York: Springer Verlag.

von Neumann, John. (1927a). "Mathematische Begrundung der Quantenmechanik." Göttinger Nachrichten (1927): 1-57. (As reprinted in von Neumann (1961). Pages 151-207.)

-----------------. (1927b). "Wahrscheinlichkeitstheoretischer Aufbau der Quantenmechanik." Göttinger Nachrichten (1927): 245-272. (As reprinted in von Neumann (1961). Pages 208-235.)

-----------------. (1932). Mathematische Grundlagen der Quantenmechanik. Berlin: Springer. (Reprinted as Mathematical Foundations of Quantum Mechanics. (trans.) R.T. Beyer. Princeton: Princeton University Press, 1955.)

-----------------. (1961). Collected Works. Volume 1. (ed.) A.H. Taub. Oxford- London- New York- and Paris: Pergamon Press.

Newton, Isaac. (1726). Philosophiae Naturalis Principia Mathematica. 3rd ed. London: Royal Society.

Poincaré, Henri. (1890). Électricité et Optique. Volume 1. Paris: Georges Carré.

---------------. (1895). "A Propos de la Théorie de M. Larmor." L'Éclairage Électrique 3: 5-13, 289-295; 5: 5-14, 385-392. (As reprinted in Poincaré (1954). Pages 369-426.)

---------------. (1900). "La Théorie de Lorentz et le Principe de Réaction." Archives Néerlandaises des Sciences Exactes et Naturelles (2nd series) 5: 252-278. (As reprinted in Poincaré (1954). Pages 464-488.)

---------------. (1902). La Science et l'Hypothèse. Paris: Ernest Flammarion. (As reprinted as Science and Hypothesis. (trans.) W.J. Greenstreet. New York: Dover Publications, 1952.)

---------------. (1906). "Sur la Dynamique de l'Électron." Rendiconti del Circolo Matematico di Palermo 21: 129-176. (As reprinted in Poincaré (1954). Pages 494-550.)

---------------. (1908a). "La Dynamique de l'Électron." Revue Générale des Sciences Pures et Appliqués 19: 386-402. (As reprinted in Poincaré (1954). Pages 551-586. Translated as Part III of Poincaré (1908b). Pages 199-250.)

---------------. (1908b). Science et méthode. Paris: E. Flammarion. (As reprinted as Science and Method. (trans.) Francis Maitland. London: T. Nelson and Sons, 1914.)

---------------. (1954). Oeuvres de Henri Poincaré. Volume IX. Paris: Gauthier-Villars.

Schrödinger, Erwin. (1944). What is Life? Cambridge: Cambridge University Press.

Shimony, Abner. (1978). "Metaphysical Problems in the Foundations of Quantum Mechanics." International Philosophical Quarterly 18: 3-17.

--------------. (1980). "The Point We Have Reached." Epistemological Letters 26: 1-7.

--------------. (Forthcoming). "Contextual Hidden Variables Theories and Bell's Inequalities." To be published in The British Journal for the Philosophy of Science.

Stein, Howard. (1970). "Is There a Problem of Interpreting Quantum Mechanics?" Noûs 4: 93-103.

-------------. (1972). "On the Conceptual Structure of Quantum Mechanics." In Paradigms and Paradoxes: the Philosophical Challenge of the Quantum Domain. (University of Pittsburgh Series in the Philosophy of Science, Volume V.) Edited by Robert Colodny. Pittsburgh: University of Pittsburgh Press. Pages 367-438.

-------------. (1981). "'Subtler Forms of Matter' in the Period Following Maxwell." In Conceptions of Ether: Studies in the History of Ether Theories, 1740-1900. Edited by G.N. Cantor and M.J.S. Hodge. Cambridge: Cambridge University Press. Pages 309-340.

van der Waerden, Bartel L. (ed.). (1967). Sources of Quantum Mechanics. New York: Dover Publications.